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INFANTILE INCUBATION.

ITS SUCCESS SANITARILY, HUMANELY AND MEDICO-LEGAL,
OR FORENSIC VIEW, WITH CASES TO ILLUSTRATE.

BY J. J. CALDWELL, M. D., F. S. S. C., Baltimore, Md.

Dear Editor: The following report is submitted as being all-important to the culture of infants, viz., the heir expectant and the heir apparent, and the preventing of substitution of heirs infantile where large heredities are concerned.

The above is the forensic point. From a humanitarian standpoint you would make the following statement, i. e., as to which is most conducive to propriety—the poor mother tied up by penury, which calls for her daily labor from home to the neglect of her offspring, and who has the good fortune in the large cities of

*Dedicated to the Mothers' Congress.

Europe to be able to place her child "en nourrice" each day as she passes the "Maison de Nourrice," and leaves her babe and her penny, and takes her check therefore, or among the Anglo-Saxon poor mothers, who, to insure quiet and repose, keep their children under the influence of some form of opiate, while the fashionable Teuton mother secures rest for her babe with Hyoscyamin, while she returns her round of calls, or the American mother, who with Mrs. Winslow's soothing syrup or Darby's carminative feels safe in her shopping expeditions for the entire morning, and the poor laboring woman

feels that she has done a good deed with the few drops of paregoric which will insure sound sleep during her hours of work away from home. Thus each country brings up a generation of "fiends" and "perverts." Now, why should there not be some humane "Maison de Nourrice" in our city?

I would respectfully submit the following letter from a grateful mother, showing what is possible with a little trouble to enable us to effect much towards the

INCUBATION OF INFANTS.

Dear Dr. Caldwell: My little daughter G—, who is eleven years old, was born before the time. When she came into the world she was extremely small, and she had neither finger nor toe nails. She was put in a large dish and wrapped in cotton, and the dish was set in a pan of hot water, the water being always kept at the same temperature. She slept nearly all the time, and would only wake to be fed. She was so small she could not use the bottle, and had to be fed with a cloth dipped in Horlick's malted milk and put between her lips. In that way she was nourished, and after two months were over she grew like other babies, and you could not find a stronger or more healthy girl.

The process we used is similar to the incubators used now for infants. Respectfully,

Mrs. G. C.

"The immense success which has attended the artificial incubation of chickens in France," says a Scotch medical (Glasgow) paper recently (1880-82), attracted the attention of Dr. Tavernier, a learned and ingenious physician of that country. He was attached to a hospital for foundlings, and, grieved at the large number of foundlings who die within the first six months of their lives, resolved to try what "artificial incubation" would accomplish if applied to infants. He accordingly had constructed a child incubator on precisely the model of the chicken incubator. It was a box covered with a glass slide, furnished with a soft woolen bed,

and kept at a temperature of 86 degrees Fahrenheit by the aid of hot water. He selected as the subject of his first experiment a miserably-formed infant. This infant was placed in the incubator, provided with a nursing bottle and kept in a dark room. To the surprise of the doctor it ceased to cry on the second day after it was placed in the incubator, and although it had previously been a preternaturally sleepless child it sank into a deep and quiet sleep. The child remained in the incubator for about eight weeks, during which time it never once cried, and it never remained awake except when taking nourishment. It grew rapidly, and when at the expiration of sixty days it was removed from the incubator it presented the appearance of a healthy infant of at least a year old. Delighted with the success of the experiment, Dr. Tavernier next selected an ordinary six-months-old infant addicted to the usual pains and colic, and exhibiting the usual fretfulness of French infants. The child conducted itself while in the incubator precisely as its predecessor had done. After a six months' stay in the incubator it was removed and weighed, and was found during this period to have doubled its weight. It had become so strong and healthy that it resembled a child three years old, and it could actually walk when holding on to a convenient piece of furniture. These two experiments satisfied Dr. Tavernier of the vast advantage of artificial child incubation. He immediately proceeded, with the permission of the authorities of the hospital, to construct an incubator of the capacity of 400 infants, and in this he placed every one of the 360 infants that were in the hospital on the 10th of February last.

With the exception of one who had died of congenital hydrocephalus, and another who was reclaimed by its repentant parents, the infants were kept continuously in the incubator for six months when they were removed in consequence of having outgrown their narrow beds. The result will seem almost incredible to persons who are unfamiliar with the reputation of Dr. Tavernier and

have not seen the report made in 1880-82, Paris, France, to the French Government on the subject by a select committee of twelve. The average of these infants' ages last February was eight months and three days, the youngest being less than twelve hours old, and the oldest being not older than eleven months; their average weight was sixteen pounds, only one of the entire 360 having attained a weight of thirty-five pounds. At the end of six months of artificial incubation the average weight of each infant was twenty-four pounds, and there was not one that would not have been supposed by a casual observer to be at least three years old; in other words, six weeks of artificial incubation did as much in the way of developing Dr. Tavernier's foundlings as three years of ordinary life would have done. Now some of this extra flesh may not have been good fat, but the point is that the children did live, did thrive and did fatten rapidly by this inactive mode of nourishment in a heated atmosphere. We might cite the proverbial goose which was honored above his fellows in the flock by being taken in hand to furnish the famous *pate de foie gras*, for the epicure's table, where by being confined in a box into which he fits closely, with his neck only out, he is fed or incubated to his full satisfaction, the air of the room being kept to a high temperature. The bird is crammed daily with mush mixed with fat until his liver takes on a large amount of fatty matter, when he is killed, and the fatty liver is cooked and sliced up to be mixed with the *pate (pie)* material. We all know how inaction favors the deposit of fat in animals. We wish to fatten an ox—we soonest accomplish this by confining him in a clean, close stall, the warmer the better, giving him all the soft food or new corn he will eat, and all the fresh clean water he will drink. It is estimated that the animal will thus take on from one to two pounds a day, and thus keep ahead of other cattle which are fed on hard corn, hay, and which wander over the fields. Just so in London. In Leicester Square was once, and may

still be, for all we know now, a large incubator where parties could get the eggs of their pet birds hatched. Miniature births were arranged along the wall of small rooms kept at 100 degrees, and every and each set of eggs was labeled and nursed by persons employed for the purpose—the hen egg for 21 days—the duck and goose egg longer. Even eggs were left there which had been brought from their native heaths, such as the paradise bird's and the ostrich's eggs. This then was regarded as a great curiosity; but now it is considered the cheapest way to raise chickens for early market. In Paris for a great while have been small "*maisons de Nourrice*" for little children scattered through the city, where mothers going out to work could leave their babes for the day. This was not connected with any foundling establishment, but was the result of a limited charity, where charitable individuals wished to aid, and yet not support, the children and mothers. The poor mothers would nurse their babes at the "nurse house" there and leave it to be fed and cared for until she had completed her day's work, probably a mile away, when she would return to find her babe clean, well fed and happy. For this accommodation she paid from one to five sous each day that she left her babe. This was a sort of post-incubator so to speak, and often it would have been better for the little one not to have been born (incubated), if there were to be no helping hand for its sustenance afterwards. Then they have the "*Hospital des Enfants Malades*," where sick children were taken to be treated until they were well, or dead, for many children die when sent to hospitals surely. They are there "*nobody's darlings*," and are lucky if they live at all. Then there are what are called "*tours*," which work like boxes on a pivot, which are turned so they will project out of the window when the bell is rung. The new-born babe is laid in this box, with, maybe, some mark to identify it in after years, and this is the "*Hospital des Enfants Trouves*" (foundling). Since the establishment of these *tours* or baby boxes, the

crime of infanticide lessened greatly in France. But the so-called creches or nurseries most resemble incubators in keeping fed and warmed the new-born babe. In these creche (cradles) maisons if the child is one year old it may have a cradle to itself in the form of a little basket. These places open early in the morning and close after dark. Up to 1852 the number of babies who enjoyed the benefit of these cradle establishments is said to be 11,000. Now that the population is so greatly increased, naturally there are more such establishments. A physician attended each creche hospital (maison) daily, and the poor mother was thus afforded a means of caring for her offspring and of performing her work besides. Give a new-born babe quarters, warmth and food, and it will thrive well, barring colics and contagion.

In this connection we would call the attention of such as have the burthen of rearing young infants to the great importance of selecting first the kind of food essential to the ready, easy and complete nourishment of such feeble individuals. We need a food not all starch, as some of the advertised foods really are, and one which does not require to be enriched with cow's milk, but one which combines all the advantages of the best infants' food without any of the objectionable features seen in so many now on the market, and we have long since settled on Horlick's malted milk as the food par excellence for babes. Its freedom from starch renders it a food which is readily digested, and it appears to be properly malted without being burnt. It is strong enough in sugar, dextrine, fats and albuminoids, without having the tendency to produce starchy indigestion.



NOTES ON SOME OF THE CLINICAL FEATURES OF TUMORS.
THEIR ANATOMICAL CHARACTERS, MORPHOLOGICAL ELEMENTS AND THEIR THERAPY, BY TENTATIVE, CONSTITUTIONAL OR RADICAL MEASURES.

BY THOMAS H. MANLEY, M. D.
NEW YORK.

The subtle and mysterious atomic changes incident on the evolution of a new growth, a true tumor, quite staggered the great philosophic mind of John Hunter. In vain and repeatedly he sought to penetrate the difficulties in the way, but without effect; and hence, in the voluminous work of the extraordinary investigator, it will be observed, that when dealing with the pathology of tumors, an obvious reticence is observed or opinions are expressed with reserve.

Virchow's great work, from which so much was promised on this topic, in its bearing on cellular pathology, has given us nothing, and was most disappointing; when the illustrious Heitzman demonstrated that the cellular theory, cellular multiplication, a regeneration from pre-existing cells, was a myth.

Conviction on this point was brought home to the sceptical, when, in the case of the late, lamented Crown Prince of the Germans, who was seized with malignant disease of the larynx, and so great an authority as Virchow, himself, in an examination of a clipping of the growth, pronounced it a "papilloma," or, in other words, confounded a wart with an epithelioma. Vast and exhaustive has been the study of the neoplasmata during the

latter half of the present century, and while little or no progress has been made in widening our knowledge of those of the cancerous group, yet the advance in the way of isolating, differentiating and classifying them has been great. By this means, we have been enabled to weed out, as it were, or cast aside, as essentially benign, very many types of hypoplasia, formerly mistaken for malignant; for example, actinomycosis, tuberculous ulceration in the passages, syphilis, and other specific or simple forms of the granuloma or an infectious origin.

The words "tumor" or "cancer" convey a harrowing impression to the unhappy victim.

Let us designate them "gatherings, enlargements or swellings; in fact, any by term implying ambiguity until, by a proper examination and observation of the case, we are assured with certainty that the case is one demanding the intervention of surgery, or active measures of any description.

A definite examination of this class of cases in our time implies very much more than formerly. This fact cannot be too strongly emphasized, inasmuch as now we are in possession of quite definite and absolute resources of diagnosis, never utilized or understood until recently.

The mechanical exploration of the passages, transillumination, the Roentgen rays, microscopical analyses of the corpuscular elements, etc., with a searching inquiry into the symptomatology and the clinical aspects of the case, all are free to flash the path and clear the way of many obstacles and barriers in the way of grasping the salient features of the case. Sometimes, however, especially in neoplasms involving the viscera, or conditions simulating tumors, very great embarrassment may be encountered.

The writer, on his present tour in Europe, lately witnessed a striking example of this in a case which he was kindly invited to see operated on.

The patient, a middle-aged person, gave all the clinical and physical evidence of a cancerous tumor, high up the rectum. A krake had been proposed, but promptly refused. It was then decided to perform Madyl's

operation or an inguinal colotomy en deux temps. But the day after the intestine was fixed in the incision the patient had a very large, spontaneous evacuation, per rectum. Scyballae of hard consistence and great size were evacuated with immediate relief of the symptoms and a total obliteration of all the signs of the "tumor." It has been my experience to have several times noticed the singular freak of colic peristalsis, after the most persistent obstipation when an anaesthetic is freely given; and, as in above case cited, all in whom I have observed it were females.

All humanity is profoundly indebted to surgery, which will add years of life and freedom from the pain of a new growth or unchanged obstruction; but the reward is greater, if the same result may be accomplished by remedies directed to the deranged tissue metabolism, degeneration, or other causes which produce it.



Society Reports.

OBSTETRICAL SOCIETY OF CINCINNATI.

OFFICIAL REPORT. MEETING OF FEBRUARY 18, 1897.

The President, Rufus B. Hall, M. D., in the Chair.

E. S. McKee, M. D., Secretary.

DR. JAMES FRANKLIN HEADY
REPORTED

TWO CASES OF PUERPERAL CONVULSIONS TREATED WITH LARGE DOSES OF MOR- PHINE HYPODERMICALLY.

(See next issue.)

DR WILLIAM GILLESPIE READ A
PAPER ENTITLED

FORCEPS ROTATION IN OCCI- PUT POSTERIOR POSITIONS OF THE VERTEX. (See next issue.)

DISCUSSION.

Dr. Giles S. Mitchell—I will not consume much of the time of the Society. I want to congratulate the gentlemen and the society upon the elaborateness of these initial papers. Both of them are even above the high standard of the good papers presented by candidates for admission.

I was extremely interested in the recital of Dr. Heady's cases of puerperal eclampsia, and he certainly is to be congratulated upon being successful in saving two patients, who were, from his description, almost beyond hope of recovery. While I have long ceased to recognize albuminuria as an important factor in the production of puerperal convulsions, yet in the few cases that I have seen in my own practice and in consultation, an examination of the urine has never failed to reveal a very large per cent. of albumen. In this particular the two severe cases related by the essayist, so far as my own experience goes, are unique. I have never had any experience in the use of large doses of morphia in puerperal eclampsia. I know that its ex-

hibition is recommended very highly, and a proof of the judiciousness of the practice has been established in a measure by the recovery of two so serious cases that were reported by the essayist. It has been my habit in these cases to rely almost solely on very large doses of chloral hydrate. I give a drachm of chloral hydrate by the rectum, and repeat it if necessary. The accouchement force, as practiced by the doctor, the rapid dilation of the cervix and the immediate delivery of the child, I believe, at least where the os is dilatable, is judicious practice, recognizing, however, that any violence in the accomplishment of this is liable to bring on a convulsive attack. The accouchement force is also a conservative measure in the interest of the child. I believe puerperal eclampsia has no pathology, so to speak, but that pregnancy is the great physiological test of mental and bodily soundness in women, and that what would excite a puerperal convulsion in a woman with an unstable nervous system would not bring about a seizure of this kind in a woman who was perfectly healthy.

As to the last paper I said at a recent meeting of this society all I have to say about the management of occipito-posterior positions. My experience with this class of cases has not been very large, although a few years ago I had quite a large obstetrical practice. I have never seen a difficult case of occipito-posterior position. Undoubtedly these do occur, but I believe they must occur very rarely. The vast majority of them, as the gentleman has stated in his

paper, take care of themselves. And I am sure that it would require more than an ordinary amount of skill in the use of the forceps to apply them when the head is high up, in order to accomplish rotation. In this position it is desirable to grasp the head in the bi-parietal diameter. The blades of the forceps naturally follow the sacral curve, and you are almost certain to apply one blade over the forehead and one blade over the occiput, and in this way you are more likely to complicate matters than to simplify them. I believe the safer plan is to simply introduce the hand or vectis. But I have never been called upon to put this into practice at the bedside. I have seen cases deliver spontaneously, the occiput sweeping the perineum, and in two instances there was some laceration, but not serious.

Dr. Thad. A. Reamy—I would like to ask Dr. Heady what the condition of the pupil was at the time the patient was comatose before he gave the morphia.

Dr. Heady—It was contracted.

Dr. Reamy—The type of cases presented in Dr. Heady's paper, in which there is no evidence of any serious renal deficiency as to the quantity of urine that has been excreted before the attack, and no evidence of albumen or casts, and where the cases are so typically nervous in character as these were, in the incipency at least, those are the cases that are pre-eminently adapted to the morphine treatment, and the doctor was, therefore, judicious in selecting that treatment for this class of cases. Clark wrote the first paper and made the first elaborate report upon this subject, and called attention to the use of morphine, and I think it is now the custom of practitioners to use the morphine treatment in that class of cases. In a paper before the American Gynecological Society, three years ago, I reported that I had used morphine in large doses in a few cases such as have been described this evening. The clinical history Dr. Heady has given us; however, it is extremely interesting that associated with the unconsciousness there was contraction of the pupil, and that in that con-

dition the action of morphine was so gratifying. I have seen coma follow the convulsions when there was wide dilatation of the pupils. Of course, that is a very serious combination, and I have felt afraid in such cases to give large doses of morphine. I doubt if I would have had the courage to give such large doses where there was already contraction of the pupil, yet the result in Dr. Heady's cases shows the wisdom of his practice.

While I do not rely upon any one remedy in the treatment of cases in general, yet I think it is recognized that in the large majority of cases of puerperal eclampsia there is renal insufficiency. In other words, there has been an imperfect action of the kidneys. I do not mean to convey the idea that I think these convulsions are due to this; I think it was all wrong to call them uremic convulsions, but in most cases, even of nervous origin, we have albumen in the urine at this time, if we do not have it before. Without any doubt, in the vast majority of cases there is excrementitious material in the tissue, that ought to be eliminated by the skin or the kidneys, or both, and therefore any plan of treatment should be, first, to control the convulsion in which there is the greatest danger; second, to relieve the conditions on which the convulsion depends. But I do not believe, as was advocated by a few prominent speakers in a recent discussion of this subject in New York, in the rapid and prompt delivery in every case. I do not believe that is always wise. I think the class of cases in which speedy delivery should be practiced is distinctly defined; I do not believe, in a large percentage of these cases, that when you have removed the child you have the conditions upon which the convulsions depended. In one of Dr. Heady's cases the convulsions came on after the delivery, and in some of the worst cases I have ever seen that has been true. In a fatal case in the West End the woman had no convulsion until after the delivery. Indeed, other things being equal, the convulsions coming on after delivery are more likely to prove fatal than when they come on

prior to delivery. If the delivery can be accomplished, it certainly is well and desirable, especially in a nervous case, particularly when we are able to deliver with so much facility as Dr. Heady did in one of these cases. When the convulsions are coming on rapidly, the remedy above all others is the use of large doses of veratrum viride. I use this hypodermatically in doses as large as forty drops, and scarcely ever would I begin with less than thirty drops of Norwood's tincture of veratrum viride. If the woman could swallow and was still conscious, and the convulsions were not coming on very rapidly, veratrum viride could be given by the mouth and would manifest its action in twenty or twenty-five minutes. I have made it a rule to continue the use of this remedy until I have brought the pulse down to about 40 or 50. It then is rather difficult for the patient to have a convulsion, because the action of the convulsive centres is pretty well suspended. In these cases veratrum viride produces just the same condition that you would secure by large blood-letting. In this way we may secure all the good that we could from blood-letting, unless you take the ground that we get rid of improper material by blood-letting, which is very far-fetched. The advantage is that you bleed the vessel into the vessels of the viscera, by the action of veratrum viride upon the vasomotor centres, and thus you retain the blood. Another great advantage is that when the convulsions have been suspended and the immediate danger is past veratrum viride acts with great efficiency as a diuretic, and before that produces profound diaphoresis, so that in these two ways you secure the elimination of the material which is doing the damage in the blood.

Dr. G. S. Mitchell—I believe the majority of those who advocate the accouchement force, do it more in the interests of the child than to prevent convulsive seizures.

Dr. Reamy—It is advocated for that purpose. But one of the gentlemen, to whom I have referred, took the ground that the presence of the child caused the convulsion, and he

proposed to forcibly dilate and deliver. He said he could dilate and deliver in 20 minutes.

The rotation, to which Dr. Gillespie has referred, I will not discuss. It is well known that I have been in the habit of rotating with the forceps. In the first place, to use an instrument as Dr. Gillespie has described, it must be an instrument without too great pelvic curve. In the next place, the blade of the instrument must not be too thick. I suppose Dr. Gillespie is accustomed to using an Elliott forcep or a Smedley forcep, or some such instrument.

Dr. C. D. Palmer—It is unfortunate that these two valuable papers have been brought forward this evening at the same time. There is enough for us to discuss in one of these papers without touching the other. And I will not say anything about the second paper, although I think it is very commendable. I do not want the gentlemen to think I do not appreciate it.

I have never seen a case of puerperal convulsions, that I can recall, in which the urine was not albuminous. I do not mean to say that the urine is invariably albuminous before the convulsion, but if it is not albuminous before the convulsion it becomes so afterwards. There seems to be something in the convulsion that interferes with the action of the kidneys, so that the urine may become albuminous afterwards. In view of that fact, I asked the gentleman if he had examined the urine of the patients after the convulsions, and he said he had not. I think if he had done so he would have found albumen, consequent upon the convulsion. I think it is well recognized that not all these cases of convulsions are uremic. There is a consensus of opinion now that convulsions may be due to a toxemia, the kidneys, skin or the bowels being at fault, from the pregnant condition. Emotional causes, of course, will cause these convulsions. Now I take it that these two cases were such as belonged to the hysterical or epileptic variety, and not to the apoplectic variety of eclampsia, and I take it that they were exactly the class of cases in which we would ex-

perience good results from the hypodermic injection of morphia. I have never given such large doses of morphia hypodermatically as reported this evening. I usually give gr. 1-4 of morphia combined with atropine. I never have given more than gr. ss. of morphia at a dose, and I usually give it with gr. 1-100 to gr. 1-50 of atropine. In a case of puerperal eclampsia I would first think of emptying the bowels; then of acting upon the kidneys or upon the skin. There is a potent influence, which we ought not to ignore, from the administration of chloral. I think all the good effect of chloral may be obtained by giving it per rectum, repeating it if necessary. *Veratrum viride*, I believe, is the best remedy in these cases. I give it by the mouth, or, if it is necessary, hypodermatically. It is good in almost any condition of uremic convulsions, but particularly is it of value in the apoplectic form of the disease. I think the action of the morphia is aided by the *veratrum viride*. These medicines do not counteract each other, but they help each other.

Dr. Charles B. Schoolfield: I have had three cases of puerperal eclampsia. The experience of nearly all obstetricians in regard to these cases is that every case is largely a law unto itself. Each case was treated differently. One of them died. In the first case the convulsions came on after the child was delivered. The woman never came out of the convulsions and died. The second was one in which I was called in consultation. The woman was in convulsion and we bled her. She came out of the convulsion and recovered. At the time approaching her next parturition she again began having symptoms of convulsions, headache and dizziness, and a few days before the confinement I bled her freely. The headache and all symptoms of convulsion disappeared, and she went through her parturition without any trouble. About two years afterwards she again became pregnant and had the same conditions preceding the parturition. She began to have severe headache, the urine was loaded with albumen, as it was in both the other pregnancies, and I

again bled her freely at her own request, because she felt in both the other pregnancies the bleeding had helped her. She passed through confinement safely without any puerperal convulsions. The next time she became pregnant, when the time approached for her parturition, instead of bleeding her I began giving her the bromides and chloral. That relieved the headache for the time being, but her headaches continued after delivery, and her urine was not up to the normal in quantity, and was still alubuminous. I kept on giving the bromides and the hydrate of chloral. On the tenth day she had a stroke of apoplexy, with hemiplegia and coma, and died in a few days without gaining consciousness. I think if I had bled her as I had before I would have saved the woman.

In another case I gave *veratrum viride*. The patient began having convulsions about ten or twelve days before labor. After giving two or three full doses, gr. xxx, the symptoms all subsided and I stopped giving the remedy. The patient had no further symptoms until the day of labor. She then commenced to complain of headache, and fearing convulsions would come on I again gave her thirty drops of *veratrum viride*. She then went through labor without any trouble. I believe if I had given *veratrum viride* in the previous case it might have saved the patient. But the bleeding acted promptly, relieved the headache and the arterial tension, and made the condition of the patient very much better. I have never bled in any other case, because I did not think it necessary, but in cases such as that I think bleeding is an excellent thing.

Dr. W. D. Porter: I, too, regret that the evening is so far advanced when we have two such good papers before us. In the first case of convulsions I think the result was due perhaps as much to the emptying of the uterus as to the medication. The administration of large doses of morphia relieved the condition, but I think some drug which would lower the blood pressure would have acted better. While, as Dr. Reamy says, it is not desirable to use violent measures to empty the uterus, yet I think

prompt measures are desirable. I have seen but two cases of puerperal convulsions. The first, a most appalling case, occurred in the person of my own sister. I was called to that case a long distance away after the patient was unconscious fourteen hours, and had had twenty-five convulsions. The pregnancy had advanced to the seventh month. There was complete suppression of the urine. The cervix was dilatable, and the treatment consisted in emptying the uterus promptly and using pilocarpine hypodermatically. In a little while there was profuse perspiration. In addition to the pilocarpine, purgatives were also used. After this there was but a single convulsion. The condition of coma, however, remained about eighteen hours after the treatment. Then the patient gradually recovered consciousness, but was unable to sleep much for a long time. Four days afterward she had an attack of puerperal mania lasting two or three hours, which failed to yield to chloral or bromides, but finally yielded to chloroform, and after a sleep the patient made a slow recovery. Partial hemiplegia persisted for several months afterwards, gradually clearing up.

The second case I saw in consultation in the city. In that case the convulsions came on soon after delivery. The patient had three convulsions. *Veratrum viride* was used with very good results, at first, grt. xx by the mouth, and afterward the same dose hypodermatically. The action of the drug in this case was very prompt. After the use of the drug hypodermatically the pulse rate fell from 130 to 60, a diminution of more than one-half, inside of twenty minutes.

As to the occipito-posterior position being a rare condition, as was said by one gentleman, I think it is far from a rare condition. The gentleman referred in his paper to the fact that often the head remains above the brim and fails to engage, because the occiput is posterior. I think I have seen several such cases, and I think many cases thought to be cases of rigid os are really cases in which the head does not engage

promptly, and, after it does engage, descends slowly because the occiput is posterior. I do not think the gentleman recommended the use of the plan he suggested in all cases, but only would assist nature to rotate if possible, and, if not, would favor the descent of the head and accomplish rotation at the outlet. I confess that perhaps I have always been prejudicial in the use of the forceps in changing the position of the head in these cases. I think the method described by the gentleman is an excellent one, and in some respects superior to other methods of rotating with the forceps. In the first place, you can secure flexion of the head, and I do not think this can be done with the forceps at any other point than at the outlet. You can grasp the head so as to make proper leverage, and you can move the handles in a sufficiently large arc to cause flexion.

I had a case perhaps a month ago in which I tried the method described by the gentleman this evening, who had previously described it to me in a private talk. The case in which I tried it was one in which the head descended to the pelvic floor without any difficulty, and I simply applied the forceps as described in the paper, flexed and then rotated the head and delivered the child, with very slight tear of the perineum.

Dr. A. W. Johnstone: I believe we are just on the verge of finding out much we have not heretofore known about these cases. I believe paraxanthine is at the bottom of it all, and if any of you will give me at least a quart of urine from such a patient I will see that it is properly examined. The concrementitious materials are not excreted. We know now that urea is very little more poisonous than bicarbonate of soda, and it is all a mistake to call these uremic convulsions. That is a thing I have known only about six months, although to the older physiologists it has been known a few years. The hysterical, apoplectic and other varieties, I believe, are simply, like petit mal and grand mal, etc., due to the presence of some poison. No woman with puerperal convulsions ever had

as hard a time, and not die, as did one case I saw. The convulsions were simply terrible, and the coma that followed proved beyond a shadow of a doubt that it was due to paraxanthine, and I saw six or eight mice and one guinea-pig killed by the paraxanthine gotten out of a quart of urine from that patient. They were attacked by the same kind of convulsions that we saw the woman have. If there is anything in the reproduction of symptoms in toxicology, here we have it exactly. I have never seen but one case of puerperal convulsions in my life. The case was also seen by Dr. Mitchell. The patient got well very promptly and had very little trouble. Suppose these cases are due to paraxanthine, how are we going to treat them? Others have been on the right track in trying to get rid of this material in one or two ways: One is to get it clear out of the body, and the other is to drive it back into the tissues, where it is harmless for the time being. Now, all your morphine, chloral and all the anodynes, act on these compounds just as they do on uric acid. It is a well-known fact that you can stop the formation of uric acid very largely by the use of these remedies. It is very interesting to read up the control of uric acid by these drugs. But to me, if the patient was anything to me, one for whom I would do my very best, I would feel very much like bleeding her, but I would bleed in a different way from that suggested this evening. At the same time that I would make a venesection I would have a saline solution going in, as was suggested in a little article I read somewhere recently. It was found that the patients get well readily when given saline solution at the same time they were losing blood. It struck me then that the next case of convulsions I should treat upon the same plan. So far it has been found that paraxanthine is not excreted by a diseased kidney. Thus, paraxanthine was found in the blood of a man who died of convulsions in Bright's disease. Now, we want to see if we can find it in the blood of these eclamptic patients. Dr. Rachford said recently he would be glad

to examine the blood from one of these cases of eclampsia. Should any of you have such a case, if you cannot get the blood to Dr. Rachford, if you will send it to me I will see that it gets to him. Such an examination will enable us to know just what these poisons are.

As to the last paper, I have expressed my views upon that when we had the subject up before. I have had eleven cases of occipito-posterior position, of which I saved every case but one. In the case that terminated fatally the cord had been down and pulseless six hours before I saw it. With slightly curved forceps I have had no trouble turning in these cases. It is true in some cases we have to push the child up some before we can turn. But we should never be satisfied until we have the child by the back of the neck, when we can dislodge the head and push it in almost any direction we desire. In the last case I had the head was clear up above the brim of the pelvis, and I had only about half the pelvis to work in, for the right side of the pelvis was filled with a cellular swelling (that disappeared in a few months), and I succeeded in delivering the child all right. If the child that died had been living when I saw it I believe I could have saved it.

Dr. Mitchell: Did you make this rotation above the pelvic brim?

Dr. Johnstone: Yes, sir.

Dr. Mitchell: What was the occasion for it.

Dr. Johnstone: It was a case of occipito-posterior position in which the head of the child could not descend because of the swelling. I am glad to hear the doctor's test with the spinal fluid. Goodell told me of scores of cases in which he had done this way. At that time it seemed to me difficult, but I have done it in four cases.

Dr. Porter: I would like to ask Dr. Johnstone if he would not prefer to turn in such cases.

Dr. Johnstone: Well, my arm was not long enough for that in this case. The occiput was low posteriorly, and could not be budged with the hand, and so I applied the forceps and dragged it through, although it is

true it took me an hour and a quarter to do it. I have never known just what the swelling was. But even in this kind of complication it is possible to turn the head and apply the forceps right. The only difficulty is to apply the forceps after the head is in the right position. I am surprised that the method of Goodell is used so little here in Cincinnati. As for hurting the mother, if the forceps fit all right, and particularly if you have the stops in the handles and do not leave any of the blade sticking up, you will not hurt the patient at all. In none of these cases did I do more than to nick the perineum, and in that case the perineum only had to have two or three stitches in it.

Dr. Rufus B. Hall: I do not want to discuss at any length the subjects presented this evening by the essayists, for two reasons: First, as has been expressed by several speakers, either paper would be sufficient for the evening; and, second, the time is so limited.

I want to say a few words in reference to the first paper, by Dr. Heady. I do not want to say it as an apology, but it is a fact that I do not do obstetrical work now. I rise to speak for a special reason, that will be apparent. I will only emphasize some remarks made early in the evening. Let the cause of the convulsions, in some cases supposed to be nervous, be the xanthine bodies or of nervous origin, it matters not, providing we are on the right road in treatment, as suggested by the last speaker. If we are treating these cases rationally and intelligently, it matters very little to us whether or not the cause is the xanthine bodies or uremic poisons or reflex from the nervous system. The essayist certainly reported two very interesting and remarkable cases, and the results sustain the treatment. The treatment gave results better than could be expected from any treatment before it was tried. But what I arose more especially to emphasize was a remark made by Dr. Reamy in emphasizing the veratrum viride treatment in puerperal convulsions as we ordinarily see them. I do not believe in societies

like this we emphasize the importance of this treatment as much as we should, and I only want to report a case to emphasize that point. Not many weeks ago I was asked to see a case in one of the suburbs. It was a case of convulsions in a plethoric woman about forty years of age, her first confinement. I promised to see the case in consultation because they could get nobody else, and only on condition that they would release me after the first visit. I said to the doctor over the telephone: "Now, won't you give the patient a hypodermic injection of thirty drops of veratrum viride?" He asked if I wanted him to kill her. I replied that if he would promise to do that I would come. He promised to do it, but the consulting physician objected to it. I saw the patient an hour afterward and found she had had five drops. It had had no effect. The woman had had a number of convulsions, and was in a dying condition. At first neither one of them would consent to the hypodermic injection of twenty-five drops more. Finally, when the patient seemed almost dead, they allowed me to inject twenty-five drops more. In half an hour I noticed some effect. In three-quarters of an hour the patient was given ten drops more of veratrum viride. In half an hour the pulse was 80 and she was semi-conscious. Then the doctors were converted. The patient was given ten drops every hour until the pulse was down to 50. The patient never had another convulsion, and is well to-day. The reason I report this case is to emphasize the importance of bleeding into the veins, as it were. But few men have the courage to give sufficiently large doses of veratrum viride on their own responsibility. When this treatment was first suggested I thought I would as soon shoot the patient, it seemed to me so dangerous. But it is just what these patients need.

Dr. Heady: Each patient had a convulsion after delivery, and I did not give the morphia until after the convulsions appeared after delivery. Often the convulsions stop when the child is delivered. I do not want the society to believe that all my cases have recovered; not by any means.

The first case I saw was some fifteen years ago. The urine was loaded with albumen, and the patient showed general anasarca. Delivery was accomplished without convulsions, but an hour after delivery convulsions began and continued until the patient died. In that case I used morphia and chloral without effect. I think the use of morphia and chloral is good treatment in the nervous variety of puerperal convulsions.

Dr. Gillespie: As to the application of the forceps to the bi-parietal diameter, Dr. Mitchell has said that this is difficult when the head is high up. Of course, it is impossible to apply the forceps in this way at the superior strait, but it is quite different when the head is low down; but when the head is even high in the cavity it is often possible to grasp the head regularly, and we should do so for the reasons stated in the paper. I think if he were to take the trouble to apply the forceps as I have described he would find delivery much more easily accomplished. I frequently, before I began this method, had very great difficulty in delivering some cases. Another objection to applying the forceps irregularly, and one I fear I did not emphasize enough, is that if you apply the forceps with one blade on the forehead and the other on the occiput you have a much more insecure hold in the posterior than in the anterior position. If it is an anterior position and you squeeze the handles together sufficiently to keep the forceps on the head, if any rotation takes place the occiput will swing forward; but in occipito-posterior positions the tight gripping of the handles will cause the occiput to go backward. This is what first called my attention to occipito-posterior positions. The case was one in which the forceps slipped when not applied in the bi-parietal diameter. I pulled and pulled until I was satisfied the child was dead, and then I perforated with a pocket knife and got through with the delivery. Such an experience is not pleasant, and I began to think about these cases. With the head below the brim, I would prefer, if I could bring it down without great force, to bring the head down on the

perineum and rotate. Dr. Reamy referred to the pelvic curve. I do use the Elliott forcep, which has not a very excessive curve. The manipulation high in the pelvis, which I have described, however, I think could be accomplished by the double application with any ordinary forceps. By turning the head in the transverse diameter and applying the forceps in the opposite diameter you can complete rotation and delivery. But the rotation at the inferior strait or on the perineum is the easiest manœuvre I have ever executed with the forceps. The main secret is to have the forceps applied on the bi-parietal diameter, bring the blades parallel with the long diameter of the head and then flex and turn without force. The first time I delivered with the forceps in the reversed position I simply grasped the forceps, resting the handles under the forearm, and lifted the head over the perineum in order to see how the blades applied themselves to the head.

REPORT OF A CASE.

Dr. Magnus A. Tate: At the last meeting of the Obstetrical Society I presented a specimen of urine (coagulated), and I now give a somewhat detailed account of the case.

Patient, aged twenty-two, colored, well developed, primipara, seven months pregnant, entered Ohio Maternity Hospital, December 21, 1896, at 11 A. M. A complete obstetrical history would make my report too long, as it embraces many details of minor importance not necessary to relate in this instance.

Previous history: Patient had had measles and whooping cough.

Examination of urine: Specific gravity, 1022; acid reaction; no sugar; light color and a slight trace of albumen.

Case from day of entrance until time of delivery watched, and frequent hot baths, proper diet and regulation of bowels resorted to. The day previous to delivery patient had done an ordinary day's work (light work, as she was now eight months pregnant), which she had been doing for four weeks around the hospital.

On the night of January 20, 1897, patient retired, not having complained of being sick; in fact, at no time during her stay in the hospital had she even hinted at feeling sick. The next morning, at 7 A. M., patient began to vomit, and complained of violent headache. The nurse very naturally thought patient had eaten something that had disagreed with the stomach, but the vomiting not yielding to treatment, I was called and saw her at 10 A. M. When I arrived nurse reported patient very much worse, and I found her about as follows; Breathing somewhat labored, skin moist, temperature, 100 degrees; pulse, 120; pupils did not react to light readily, and it was with considerable difficulty that patient could be aroused. Immediately introducing catheter I drew off two teaspoonfuls of bloody-colored urine, which, upon testing, was found loaded with albumen, and heat gave a brown coagulated mass. I ordered patient a sweat and an enema. In an hour's consultation with Drs. Zinke and Rowe, the following course was determined upon, they agreeing with me in diagnosis. Delivery of child. The cervix not being dilated over the size of a five-cent piece, I dilated with fingers, applied forceps, delivery of child (lived six days), and placenta. No laceration of consequence. Only a few whiffs of chloroform, which was really not necessary, but merely as a safety valve to a convulsion seizure. Delivery accomplished, patient again removed to hot room and sweated, carefully watching fundus of uterus to prevent any undue hemorrhage. Hypodermics of strychnia, ether, digitalis and whisky given; no improvement followed. The pulse increasing in rapidity, veratrum viride given every hour for four hours with no diminution in pulse rate. The patient being so large and fleshy, bleeding was resorted to, but all to no avail, as she succumbed at 9.15 P. M. From the time stupor supervened twitchings of hands and face

constant, but no violent convulsive seizure.

The very interesting feature was the patient's apparently healthy condition four weeks prior to her demise; her ability to do light work, and at no time a complaint—not even a headache. The lesson taught is the great care necessary to a pregnant patient with even a slight trace of albumen in the urine, and at best often our efforts are of no avail.

PRESENTATION OF SPECIMEN.

This specimen was removed at the Good Samaritan Hospital, January 22, in the presence of a class, from Mrs. N., of St. Mary's, Ohio. She is thirty-five years old, mother of three children, the youngest three years old. Present illness has lasted for eighteen months. It began with bloody diarrhea, six months later there was a discharge of pus by the bladder, and three months ago by the rectum. Recently an abscess formed near the crest of the ilium, and was opened by her physician. It is still discharging. At the time of operation there was slight elevation of temperature, pulse 120. I first attempted to drain per vagina, but the abscess was so high in the pelvis and the uterus so immovable I was unable to do so. I then opened the abdomen to see what was to be done, and at once decided that total extirpation of the uterus and appendages was advisable. This I proceeded to do, but had only gotten the uterus freed on one side when my clinic hour expired, and being unable to secure the following one, was compelled to move my patient through the hospital halls and up two stories by the cold elevator. After this delay we finished the operation as rapidly as possible, but the patient left the table much exhausted. She, however, rallied satisfactorily in twelve hours, and so far recovered as to be able to leave the hospital day before yesterday.

Editorial

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UNNECESSARY STREET NOISES IN AMERICA.

This is the way the average Englishman looks upon the noises of American cities, and with a considerable degree of truth. These things might receive attention from the authorities and in a large measure remedied.

Efforts are being made in New York and in some other large cities in the United States to prevent as far as possible the unnecessary street noises. All over the world street noises in the large towns are a nuisance as well as a serious menace to health. Some of the noise must be endured as a necessary evil, but a great part of it might be easily abolished by judicious legislation and to the lasting benefit of the general public. The worst feature of the case is that it is not only in the day time that the pandemonium is rampant, but at night, too, when the nervous system absolutely requires rest, it is often almost impossible in a large city to obtain undisturbed repose. The matter is a serious one and will sooner or later have to be

gravely considered. The noises prevailing by day and night in New York are almost beyond belief. Englishmen are rightly prone to think that London is a noisy place, but compared with New York the American, we believe, looks upon London as a village. The noises in New York have a peculiarly jarring, irritating effect on the nerves; the booming rumble of the cable cars, the metallic rattle of the trains on the elevated railway, the jolting of the carts and heavy traffic over the rough, uneven roadways, the continual sounding of the gongs on the cars, the ringing of the bells attached to hucksters' carts—all these combine to make up an aggregation of sounds which happily can be equalled in no part of London. In all large towns unnecessary street noises of any description should be sternly repressed. It seems absurd in these days of high pressure that to the unavoidable sounds should be added those which might be stamped out without doing harm to anyone.

—Lancet.

TOXINS AND ANTI-TOXINS—A
NEW THEORY.

Calmette and Delarde (*Ann. de l'Institut Pasteur*, x, 12) have reinvestigated the natural and acquired antitoxic properties of blood serum from various animals, taking the reaction to doses of abrin as the most frequent criterion. They have arrived at the following conclusions: (1) The serum of animals naturally refractory to such toxins as they have investigated rarely possesses antitoxic properties with regard to those toxins. Thus, while fowls and tortoises resist very considerable doses of abrin their serum is totally inactive with regard to abrin. Similarly, Vaillard has proved that the fowl, although refractory to tetanus, gives a serum which is without action on tetano-toxin. Even when the serum of refractory animals is antitoxic, as in the case of the ichneumon and the hedgehog with respect to serpents' venom, the antitoxic power is always extremely feeble and by no means in proportion to the degree of immunity. There is thus no relation between the natural refractory condition of certain animals and the antitoxic power of their serums in respect of the toxins by which they are unaffected. (2) While warm-blooded refractory animals can produce anti-toxins under the influence of repeated non-fatal injections of toxins, cold-blooded refractory animals produce none under the same conditions. (3) Cold-blooded refractory animals, such as the frog, can acquire immunity against fatal doses of toxin without their serum becoming antitoxic. (4) Antiabrie and antivenomous serums can be used practically for the production of passive immunity in man and animals, and also for the diagnosis of the toxins which they oppose. The former has a very marked preventive action when applied to mucous surfaces, and hence may be turned to account in ophthalmology. (5) The active substance of antitoxic serums is not modified by certain chemical reagents, which destroy or profoundly alter the toxins. It does not alter

toxins when mixed with them in vitro. It appears to exist in great abundance in the leucocytic protoplasm of vaccinated animals, the leucocytes retaining their antitoxic powers after being thrice washed free from serum. It does not dialyse through membranes; it acts as energetically on the leucocytes of fresh animals as do the antimicrobial serums. (6) Certain substances having no specific action on toxins, such as bouillon, normal ox serum, or the serums of certain animals vaccinated against various infections or intoxications can, when injected into fresh animals, act preventively as regards other infections or intoxications. Hence immunity, natural or acquired, cannot be due to the presence in the serum of a chemical substance having the power of destroying or modifying the toxins. The true existence of a preventive substance in the serum of vaccinated animals remains yet to be proved; the author's experiments suggest that the preventive power may after all be a physical and not a chemical phenomenon. Thus they have shown that the antitoxic function is independent of immunity, since the latter can exist in the absence of the former; further, that both natural and acquired immunity result from a special property of the cells. These, according to the conditions of the surrounding mediums and their own composition, yield passively to the influence of the toxins as a bar of soft iron does to that of a magnet. When these conditions change under diverse external influences, such as the tolerance of certain poisons, the functional state of the cells is modified at the same time. This may be compared to the conversion of the soft iron into steel by tempering; the steel can preserve its magnetization and transmit it temporarily to other bars of soft iron or permanently to other bars of steel. The authors maintain that a similar physical explanation can be offered of the susceptibility or temporary permanent resistance of organisms to infections and intoxications.

CEREBRAL ABSCESSSES IN CHILDREN.

At the recent meeting of the American Pediatric Society Dr. Holt gave an account of four cases of this condition which had come under his notice. Details were published in *Pediatrics* of June 1. The first case was that of a child, aged six weeks, and this has the peculiar interest of being the earliest age at which an intracranial abscess has been described. The child was born easily and naturally, and there was no history of injury or disease. It was a fat, healthy-looking infant, with nothing abnormal until four days before death, when swelling of the left thigh commenced. On admission to hospital the child was found to be almost moribund, with a feeble, almost imperceptible pulse, and a temperature of 99.5 degrees Fahrenheit, but without coma. The thigh was incised, but no pus was found, only disorganized blood. The patient died, and examination of the brain revealed an abscess in the parietal lobe, with some recent local meningitis on the inner surface of the dura mater. The abscess cavity was 7 cm. long and 5 cm. deep, and contained two ounces of pus. The only likely source of infection was from the external ears, each of which had been punctured, and the punctures were found covered with dirty fluid encrusted with pus. The second child was three months old. The symptoms developed acutely—viz., irregular shallow respiration, retracted abdomen and fever. There was also opisthotonos in the last few days and convulsions on the last of all. There was found to be a large abscess of the right

parietal lobe, which communicated with the ventricles, and pus was found in the left petrous bone, although there was no discharge from the ear during life. The third patient was also three months old and was said to have been well and strong until seven weeks before, when it fell from the bed on to the floor. Three days later the patient cried out sharply and vomited. On admission to hospital there were emaciation, occasional vomiting and discharge of blood from the left ear. Death took place suddenly, and there were found to be meningitis of the inferior surface of the cerebellum and an abscess as big as a hazel nut. The fourth case was that of a child nine months old. On admission the patient was found to be in excellent condition as regards nutrition, but there were internal strabismus, dilatation and inactivity of pupils to light, and increased knee jerks; there was no fever. Vomiting occurred occasionally, and symptoms of irritation alternated with drowsiness. Gradually rigidity developed in the lower extremities and hands, and there were slight paralysis of the left side of the face and discharge of pus from the ear. The child died in the eighth week of the illness, and at the necropsy two abscesses of considerable size were found in the right lateral lobe of the cerebellum. These did not communicate, and no pus was found in the auditory canal. In conclusion Dr. Hott gives a short account of the different conditions in which cerebral abscess occurs in children. It is rare in the first four years of life, more common later, and the first case referred to is the youngest case recorded.





THE TREATMENT OF ASTHMA BY ELECTRICITY.

Probably few physicians have attempted to treat asthma with any resort to electric currents, although in the early part of this century Philip applied a positive electrode at the back of the neck, a negative over the epogastrium; employed a constant galvanic current (dosage unknown), and gave "decided relief to twenty-two cases."

About thirty years ago Neftel paid considerable attention to the treatment of asthma by a similar method with the positive electrode applied over the pneumogastric in the neck. By lowering the irritability of the pneumogastric he achieved a success which "far surpassed" his own expectations, but was unable to positively assert that idiopathic asthma could be cured (radically cured) by the galvanic current. Writing after some twelve years of experience Neftel concluded that without doubt asthmatic paroxysms can be kept off for an indefinite time, and that every single attack may often be checked. He found, however, that a reverse of the polarity was required in some cases apparently associated with a paralytic condition of the bronchial tubes or with an abnormal reaction of the pneumogastric nerve to the galvanic current.

Others have employed the same method and reported occasional temporary, but not permanent benefit.

Whatever may be the true pathology of asthma, it is certainly held in abeyance during the action of an electric current which has a function regulating action upon the pneumogastric or other nerves. If the asthma was due to no other cause than altered nerve functions, it is theoretically certain that electricity would not only give temporary relief, but would effect a cure.

If the asthma is maintained by a persisting cause a palliative treatment cannot produce a cure until after the cause is removed. In two cases in my own experience the asthma has apparently been reflex from uterine irritation, of which neither the patients nor previous physicians were aware. Both of the cases were young ladies, about twenty years of age, neither of whom had suspected any pelvic lesion.

Case number two was that of a young lady, whose father has been asthmatic for thirty years. About a year ago she began to have severe attacks herself, which differed materially from those of her father. Some things which aggravated him had no effect upon her, and palliatives which gave him relief did not relieve her. After trying a change of location and faithful medical prescribing she reached a point of almost despair; had lost thirty-five pounds, become very anemic, and while in at-

tendance for a month at the office of a specialist, who gave her an inhalation form of treatment, she failed so rapidly as to finally be almost unable to leave her room.

Upon being referred to "electricity" as a last resort (it seems quite natural to try electricity after everything else has failed), she managed with help to reach my office, and I put her on the static platform. She was breathing very rapidly, and had just had a severe paroxysm. Respiration very shallow, and chest full of rales on both sides. During her subsequent treatment I had numerous opportunities to verify the influence of general static electrification in regulating functions. I have counted her respirations as high as sixty per minute before treatment, and had them come down to thirty-six per minute in ten minutes. Positive electrification, with long sittings of about a half hour each, mild positive sparks to the chest and to the spine constituted her static treatment, and she rapidly became able to inflate her lungs deeply without fatigue. Her breathing became almost free from rales during each treatment, and her daily exertions took on a different character at once. Her exceedingly debilitated state at the commencement improved a little each day for three days. On the fourth day she had a severe relapse, and was up all night. This was the last severe attack she had. It was attributed to a culminating gastric derangement due to drug dosing, to which she had been subjected.

It was ascertained by careful inquiry that she observed some aggra-

vation every month, and had mild dysmenorrhea. Examination showed a lateral displacement of the uterus, with considerable venous engorgement. The cervix was drawn to the right side, and held there by a tough fibrous band. The os was slightly inflamed, patulous and emitted a slight mucous discharge. I considered it probable that the asthma and uterine condition were intimately associated, and during the three weeks prior to her next menstruation she was treated accordingly. She progressed daily, and not only gained in weight and strength, but was entirely free from wheezing most of the time. She also observed that any slight aggravation at night was readily checked by burning a pastille which had before given her very little relief. Her courses came on without her knowing it, and she not only had no pain, but a much freer flow and very much less of the usual increase in the asthma.

She is unable to recollect any illness which could account for the adhesion of the uterus. Probably both the static and galvanic currents act in the same manner in ameliorating the disturbance of respiration, but the favorable result probably depends for its permanency upon removing the disturbing cause. In the two cases above referred to the disturbing cause was evidently uterine, although in the first case no lesion could be made out. The first case had no static electricity whatever or applications to the chest. She was simply treated by galvanic and faradic currents within the pelvis and the asthma abated.



Current Medical Literature.

HEMOPTYSIS.

According to Cotton, the following are the chief causes of hemoptysis: 1. Hemorrhage from the pulmonary artery or its radicles. (a) Rupture or wound of lung from external violence. (b) Active hyperemia of the lungs--inflammatory, vicarious or induced by violent effort or excitement. The active hyperemia may be primary as regards the lungs, or may supervene or be attendant upon disease already present in them. (c) Mechanical hyperemia of the lungs, secondary to heart disease or embolism of one of the pulmonary branches, or to pressure from tumors, such as enlarged bronchial glands. (d) Neurotic division of vessels in the course of softening of tuberculous or other consolidations in destructive lung diseases--phthisis, cancer. (e) Aneurismal dilatation or simple erosion of branches of the pulmonary artery, exposed in the course of excavation of the lung or ulceration of the bronchial mucous membrane. (f) Primary atheroma of the pulmonary artery within the lung. 2. Hemorrhage from the bronchial capillaries. Capillary hemorrhage from the bronchial mucous membrane. 3. Hemorrhage from the aorta or one of its great branches. Aneurism rupturing through the lung or into a bronchus. This classification of the causes of hemoptysis is drawn by Dr. Cotton from his own clinical experience and observations in the dead house.

—Med. Times and Hospital Gazette.

ARTHRITIC GOUT.

Sir Willoughby Wade divides cases of gout appearing in connection with the joints into three classes: First, the acute attack of

regular gout; second, chronic gout—possibly a sequel of an acute attack, with much deposit of urates in a solid or semi-liquid state. In this, with more deformity, there is much less pain and tenderness, though along with these redness and superficial swelling may from time to time be superadded with an increase of deposit within the joint. Third, pains referred to the joints of a more or less fugitive character, or tenderness felt chiefly on use, which latter may, if excessive, be followed by increase of pain and even some redness and swelling, or an attack of the first variety. In an attack of gout which has affected the metatarsophalangeal articulation of the great toe, lines of tenderness can be detected running from the joint across the dorsal and plantar aspects of the foot in the line of the nerves supplying the joint. This nerve affection sometimes has the character of a neuritis and at other times that of a neuralgia, and may, therefore, be described as a neurosis. The lesion may arise from injury or from the presence of some toxic agent in the blood. A neurosis is the primary and essential element. If a sensory nerve is attacked we get pain and tenderness; if a vaso-motor nerve we get redness and swelling; if the nutritive nerve of a joint we get changes in the joint, one result of which may be a deposit of urates. It may be asked how a toxin (uric acid) affects now an internal, now an external, sensory nerve; now a motor, now a vaso-motor; now a trophic nerve, and now an excretory one. This condition causes us to hesitate before we declare positively that gout is dependent upon uric acid as a cause.

—Med. Times and Hospital Gazette.

THE BABY'S BATH.

The hand is an unsafe guide by which to determine the temperature of the water. This should always be determined by the thermometer. A bath thermometer should, therefore, be a part of the equipment of every nursery. This consists of an ordinary thermometer of large size, set in a woden case. The following table shows the proper temperature of the bath for various periods during the first two years:

At birth100 degrees F.
During the first month.97 degrees F.
One to six months.....95 degrees F.
Six to twelve months..90 degrees F.
One to two years.....86 degrees F.

—N. A. Practitioner.

DILATION OF THE STOMACH IN INFANCY.

It is well known that dilation of the hollow viscera is a morbid condition commonly gradual in its development and equally, if not more, slow in its amendment. It derives, moreover, a peculiar interest from the fact that it constitutes a form of change which marks the transition between functional and organic disease—a stage at which overwork and weakness of tissue account for the whole pathology, and which is therefore often well within the hope of a healthy revision. Additional light has recently been shed on this subject by a paper published by Dr. Comby in the *Journal de Clinique et de Therapeutique Infantiles* of July 1, 1897. The writer's observations have been limited to the study of gastric dilation during infancy. He finds that at an age when the newly formed organ might be expected to be fully competent the stomach frequently shows a marked degree of dilation. In 64 children who died in the Hospital for Sick Children, Paris, this viscus was found to be dilated, its volume varying from 200 c. c. to 3100 c. c. In sixteen others it was contracted. Dr. Comby explains the former condition as being due to atony produced by an auto-infection by putrescent matters resulting from an improper diet, the latter as an evidence of the refusal of such food

by the child, who is consequently starved. He believes that infantile dilation is by no means always recovered from, and that many cases of a like kind which are discovered only in the adult or the aged have had their true beginning in the earliest months of life. As the best means of treatment besides a suitable diet, he advocates irrigation of the stomach, a catheter being passed along the gullet into the stomach, and this organ repeatedly washed out with fifty or sixty grammes of tepid boiled water or Vichy water. He has never known this method to give rise to any serious inconvenience.

—Lancet.

THE TRANSMISSION OF TUBERCLE.

Jackh has recently worked at the investigation of the question whether the sexual glands or their secretions contain virulent tubercle bacilli. In his inquiry he used the testicles and the contents of the seminal vesicles, as well as the ovaries of tuberculous patients who had died either of chronic pulmonary tuberculosis or of general miliary tuberculosis. Portions of the sexual organs or of the semen were introduced into the abdomen of guinea pigs and rabbits. Of five cases in which portions of the testicle or of semen were injected positive results were obtained, thrice with the semen and once with the testicular substance. All the rabbits remained healthy. Of three injections with ovary one gave a positive result. Examination of the young of tuberculous female guinea pigs gave only one positive result. It appears, therefore, that the semen may contain virulent tubercle bacilli, and that transmission of tubercle from mother to child is not the general rule.

—Med. Times and Hospital Gazette.

THE TREATMENT OF TYPHOID FEVER.

S. Frankel has recently written upon the treatment of typhoid and phthisis by guaiacol-carbonate. Its use in the former disease was first drawn attention to by Dr. Holscher

in 1894, and recently Dr. Eschle has made a very exhaustive study concerning the conditions governing the absorption and excretion of guaiacol and guaiacol-carbonate, at Professor Baumann's laboratory, at Freiburg. The results of the work done upon the subject may be summarized as follows: Both the absorption and the excretion of pure guaiacol occur very rapidly. Guaiacol-carbonate is disassociated and rendered capable of being absorbed only by putrefactive processes under normal conditions, therefore when there is intestinal putrefaction the duration of the excretory process does not differ markedly from that of the pure guaiacol. Hence, and also from the further fact that by the long continued use of guaiacol and guaiacol-carbonate in medicinal doses in man, and even in toxic doses in dogs, the increase of the ethereal sulphuric acid at the expense of the sulphuric acid is constant in amount, there can be no cumulative action of the drug. And guaiacol-carbonate seems to be the more thoroughly used up the smaller and more frequent the doses in which it is given. The explanation of this is to be found in the fact that the administration of large doses interferes with intestinal putrefaction and prevents the decomposition of the carbonate. In phthisis, therefore, Eschle recommends administering a dose of 4 1-2 to 7 1-2 grains of the carbonate four times daily. After very large doses of guaiacol there appears in the urine an organic body of as yet an unknown nature. Hydrochloric acid precipitates it in tough, mucous-like flakes, and it might thus occasion occlusion of the uriniferous tubules and stoppage of the function of the kidneys, thus fatally damaging the organism. This body was never observed when guaiacol-carbonate was administered, proving, as Seifert has already shown, that whilst guaiacol in large doses is poisonous, guaiacol-carbonate is not so. Drs. Hall, Greene, Shields and other physicians of the United States agree that guaiacol-carbonate is most useful in the treatment of typhoid fever. Dr. Greene claims that by its use ty-

phoid is reduced to two periods—first, one of rapid reduction of temperature; second, a period of convalescence. Tympanites, when present, disappears; dry tongue and delirium are prevented; diarrhea ceases; pyrexia stops by the tenth or twelfth day, and the patient at once enters upon a favorable convalescence. The results are probably brought about by both intravascular and intestinal disinfection. The characteristic typhoid stools become more consistent and lose their offensive odor. The smell of creosote can be detected in both the urine and the feces.

—Medical Times and Hospital Gazette.

CHLOROSIS.

An editorial in the British Medical Journal of July 24 on the subject of chlorosis treats of the Lloyd Jones new theory of this disorder, which we reproduce as follows:

"Chlorosis had been studied so long and so exactly that it had passed into the commonplace. But a new method always produces a new theory, so complex is nature, and we so simple, and thus Roy's invention for weighing a drop of blood has given, in Lloyd Jones' hands a new meaning to chlorosis. After examining 1400 healthy persons he has found that in males the specific gravity of the blood rises rapidly from infancy until the age of 17, remains stationary during middle life and falls slightly in old age, while in women it rises until puberty, but falls from that time until the age of 45, and rises from this until old age, when it again falls a little. Persons of fair complexion have lighter blood than those who are dark. The weight of blood varies with the quantity of hemoglobin, and less closely with the number of cells.

"After many researches into natural conditions he turned to disease, and, among others, to chlorosis. This he found to be characterized by a very light blood, containing little hemoglobin and few cells. 'Nothing,' he says, 'is more characteristic of chlorotic blood than extreme diminution of the volume of red blood corpuscles, together with an

equally marked reduction in the amount of hemoglobin.' In this he is at variance with other authorities, for it is usually allowed that the condition referred to is rather that of pernicious anemia, and that in chlorosis the hemoglobin is reduced in far greater proportion than the cells. He obtains his results not by counting the cells, but by measuring their volume after whirling the blood centrifugally, and this method may not compare strictly with the other. This question will have to be re-examined, but that the blood is much lighter than natural is the more important result of his researches, and is antecedently probable if its weight vary with the hemoglobin.

"Chlorosis appears to have some connection with child-bearing, for it is rarely, if ever, seen in men, and closely follows the age of puberty in women. This has long been remarked, but Lloyd Jones' observations on healthy females led him to the conclusion that chlorosis is but an exaggeration of a condition which is natural during the child-bearing period, and which may therefore be considered to have some favorable influence on fertility. During the whole period the blood of women is not only lighter than that of men, but even lighter than that of girls below 14, and the difference is most marked in women between 25 and 35 years

"The idea received a curious confirmation from the statistics of light and dark complexioned persons, which showed a distinctly greater fertility in the families of the former, whose blood we have seen to be the lighter. Lighter blood seems therefore to mean greater fertility, and while all women undergo this change to some extent, chlorotic women carry the tendency to excess. This is the novelty and the interest of Lloyd Jones' research.

"A few cases of primary anemia occur in young men, but these have remarkably heavy blood. From this and one or two other facts he believes that in chlorosis there is an excess of plasma, but that in these heavy-blooded cases the plasma is relatively deficient. He calls this condition 'oligemia,' an old term with a new meaning. But his conclusions are a little weakened in that he allows that the two conditions may be combined. Of these mixed cases we must hear more.

"With these views it is natural that he should hope to explain chlorosis by some auto-intoxication from the generative organs. The ovaries have failed him hitherto, and he is now examining the uterus. He has no difficulty in showing that the stomach and the intestines have not yet been proved guilty. Whether he will have more success with the organ now accused remains to be seen."

Current Surgical Literature.

T. H. MANLEY, M. D., New York, Editor.

OPERATIVE TREATMENT OF CHRONIC INFLAMMATION OF THE MIDDLE EAR.

Malherbe (Rev. de Chir., June, 1897) having observed much improvement of hearing after free exposure and scraping of the mastoid antrum and auditory meatus in cases of chronic suppuration of the middle ear, has been led to apply a similar treatment with certain modifications to patients suffering from dry chronic otitis. Five cases are reported in which the results of this treatment proved so satisfactory as to favor the assumption that exposure of the antrum and middle ear is indicated in cases of non-suppurative chronic otitis, which has not been relieved by other means. The main objects of the operation advocated by the author are free exposure of the mastoid antrum, which usually, in cases in which such treatment is indicated, is contracted and surrounded by eburnated bone; and removal by gorge and mallet of the bone between this cavity and the middle ear. Any adhesions that may be found are carefully divided, so that the chain of ossicles may be set free. No attempt, it is stated, should be made to detach the base of the stapes from its normal connections. The following rules have been suggested to the author by the experience hitherto acquired of this treatment: Operative treatment, he holds, should not be attempted on patients of advanced age. In cases in which the operation is indicated it should be performed early, before deafness has become pronounced and subjective noises have increased to such a

degree as to be almost intolerable. It would be useless to operate in cases in which there is no longer any cranial perception of sound. One ear only, and that in which the trouble is more severe, should be treated at the first operation. Experience has shown that an operation on one ear may be followed by an improvement on both sides.

THE RISK ATTENDING FORCIBLE CORRECTION OF DISTORTION DUE TO HIP-JOINT DISEASE.

Sainton (Revue d'Orthop., No. 1, 1897) states that in cases of faulty position of the thigh, with complete ankylosis of the hip, osteotomy near the upper extremity of the femur is the safest and most effectual method of treatment. If in cases of distortion and stiffness following hip-joint disease, there be still some movement at the joint, and the displacement and rigidity be due to contraction of muscular and ligamentous structures, the limb may usually be restored to a correct position by simple manipulations whilst the patient is under the influence of an anesthetic. This plan of treatment, however, is held to be a dangerous one by the author, who reports five cases in which, after an apparently successful result death occurred from tuberculous meningitis in the course of two or three months. The relation of the fatal meningitis to the surgical treatment is suggested by the fact that this event occurred in no less than 19 per cent. of 27 cases, in which Kirmisson restored the normal position of a limb distorted after

hip-joint disease by a bloodless and not very forcible procedure. The danger of meningitis and other results of tuberculous infection is due, the author believes, to the breaking up of encysted caseous deposits about the seat of the old articular disease.

OPERATIVE TREATMENT OF EXOPHTHALMIC GOITRE.

Schulz (Berl. Klinik, June, 1897) reports 14 cases of Basedow's disease under the care of Kummell, which were treated by partial removal of the enlarged thyroid body. In most of these cases the symptoms of this disease were very severe, and rendered life almost intolerable. Twelve of these patients, it is stated, were completely cured and enabled to resume their occupations. In the two remaining cases the operation was followed by much improvement, and there is every probability of the exophthalmos, the sole persisting symptom of the disease, disappearing in a short time. There could be no doubt, the author asserts, that each of these 14 patients presented well-marked and very decided symptoms of Basedow's disease. Frequent observation of the patients after operation during intervals varying in the different cases from two to seven years have convinced the author that objections to partial strumectomy on the ground of probable relapse, do not hold good. In one case only was there observed any renewed enlargement of the thyroid. In all the others the remaining portion of the gland showed a tendency to shrink rather than to increase in size.

RESECTION OF THE STERNUM FOR TUMORS.

Keen (Med. and Surg. Reporter, March 27, 1897) reports two cases of successful resection of the sternum for tumor, and adds a table of 17 previously reported cases. The first of the author's cases was one of resection of the manubrium, inner third of the left clavicle and lower third of the left sternomastoid mus-

cle for sarcoma. In the second case—one of carcinoma of the left breast and secondary carcinoma of the sternum at the junction of the manubrium and gladiolus—a considerable portion of the sternum was removed until apparently healthy bone was reached. The patient recovered from this operation, which was a very serious one, as it was necessary to remove at the same time the right breast, together with a layer of the pectoral muscle, but death resulted from recurrence six months later. The prognosis of operative interference in cases of tumor of the sternum, for immediate recovery at least, is good. In one of the 19 cases tabulated by the author the result was not stated; of the remaining 18 four died, a mortality of 22.2 per cent. It is pointed out that in cases of tumor—frequently sarcoma—adhesions to the retro-sternal tissues, and especially to the pleura and pericardium, do not take place until late. These tissues are usually pushed before the disease and the tumor; therefore, if the operation be not delayed too long, it may be separated from them with ease. The chest necessarily becomes narrowed when any large portion of the sternum is resected, but this seems to result in relatively slight inconvenience, either as to respiration or from loss of support of the arm.

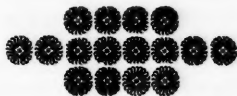
FRACTURE OF THE NECK OF THE FEMUR IN CHILDHOOD.

Whitman (Annals of Surgery, June, 1897) reports 10 cases of fracture of the neck of the femur in children between 2 1-2 and 8 years of age, and presents two skiagraphs showing the consequent deformity of the bone at the seat of injury. In the course of his paper the author deals with practical difficulties in connection with this injury, especially its immediate diagnosis, and its differential diagnosis from hip disease during the stage of recovery. It is held that the traditional age limit for fracture of the neck of the femur does not exist, and that this injury not only does occur in childhood, but is probably not an uncommon accident, which may be recog-

nized, or may at a later period be mistaken for and treated on the diagnosis of hip disease. In opposition to the accepted teaching that the symptoms here regarded as those of fracture are due to separation of the epiphysis of the head of the femur, the author holds that it would be rather extreme violence followed by non-union of the fragments or subsequent disability that would favor the diagnosis of the latter form of injury. The less the violence and the less the immediate disability the greater would be the probability of fracture when signs of fracture are present, because a bone is more likely to break in its weakest than in its strongest part, and because rapid union and almost normal functional capacity associated, as was observed in all the author's cases, with upward displacement of the trochanter, prove that the solution of continuity could not have involved the articulating surface of the femur. It is stated that this fracture in childhood is peculiar in the fact that although the immediate results are extremely favorable, even without treatment, the final outcome is likely to be a disability even more decided than after fracture in adult life.

NEW INCISION FOR NEPHRECTOMY.

Abbe (*Annals of Surgery*, June, 1897) has recently reported to the New York Surgical Society a case of hydro-nephrosis in which the kidney was removed by an unusual method. An incision was made from a point one inch inside the antero-superior iliac spine, upward and backward for four and one-half inches in a line parallel with the fibres of the external oblique muscle. After separation of the fibres of the abdominal muscles, as is done in the McBurney method the peritoneum is freely exposed, and can be readily stripped from the front of the kidney. By using good retractors the surgeon can now inspect the pedicle and separate the ureter, which can be followed to the brim of the pelvis. This method, it is held, allows nephrectomy in the most advantageous position for both patient and operator. It gives free access to the kidney and ureter, and is almost a bloodless method, as no muscles are cut across. As each abdominal muscle is divided in the direction taken by its fibres the deep portions of the wound readily fall together, and there is much less tendency to the development of hernia.



Current Literature in Obstetrics and Gynecology.

EFFECTS OF CAUSTICS ON THE UTERUS.

Labusquiere (*Ann. de Gynec. et d'Obstet.*, June, 1897) reviews the experience of several recent writers. Hofmeyer reports a case where a 50 per cent. solution of chloride of zinc was injected into the uterus. The nozzle of the syringe was very fine, almost as thin as that of a morphine injector, and the os had not been dilated before treatment. The patient walked home, but died with symptoms of acute peritonitis within 12 hours. The body was disinterred for a judicial necropsy 20 days later. There was no doubt strong evidence of peritonitis; the serous investment of the uterus and its serous folds were inflamed. On the other hand, though the endometrium was represented by an eschar, there was no such appearance in either Fallopian tube. Hence the inflammatory process had extended through the uterine wall, whilst none of the fluid caustic had passed along the tubes. Hofmeyer condemns the chloride solution; he even thinks that it acts as a poison. He refers to Duhrssen, who advises its application by means of a Playfair's probe; but Hofmeyer finds that most of the caustic on that instrument is lost on the cervical mucosa; for that reason the advocates of chloride erroneously believe that it can be freely applied to the endometrium with impunity. A needle-like nozzle should not be recommended for intrauterine injections, as it encourages the evil practice of injecting

without preliminary dilatation of the cervix. Much can be injected through a fine nozzle, but that much cannot always escape freely through the os, and the os internum may contract behind it. By dilating the cervix, using a syringe with a fair-sized nozzle, making sure that the fluid can escape from the cervix and keeping the patient in bed, strong caustics may be used with advantage. Brose thinks that even with these precautions intrauterine injections of any fluid may set up perimetritis. Schroeder recorded that complication when only solution of tincture of iodine had been employed. He advises that a Playfair's probe be used for the application of the chloride solution, which in particular should never be injected. In leucorrhea, and even gonorrhea, the cervix should be treated first. Duhrssen, on the other hand, is in favor of the injection of strong caustics, but he insists on washing out the uterus with water both before and after the injection of the caustic. He packs the uterus with iodoform gauze on the day before cauterization, so that the cervix is dilated, and in consequence the action of the caustic will be less painful. Sanger condemns both the 50 per cent. solution of chloride of zinc as injection and Dumontpallier's treatment with crayons of that salt. The chloride is an excellent therapeutic agent, but should be used like nitrite of silver and applied by aid of an ordinary caustic holder. Strong solutions of other salts should be applied in the

same way, but never injected. Labusquiere notes that Worth and others have recently shown that destruction of the endometrium may not necessarily cure even pure endometritis. In a case where after the use of Dumontpallier's crayons the uterus was removed, it was found that in place of the eschar was a perfectly reproduced endometrium. Unfortunately its pathological changes were reproduced as well. Hence much remains to be learned about the rational treatment of endometritis.

LATENT RUPTURE OF UTERUS IN LABOR.

Backer (*Monatsschrift f. Geburtsh. u. Gynak.*, March, 1897) relates the case of a woman in her third labor, who seemed to be doing badly, when it was found that the retardation of delivery was rupture of the uterus. This was surprising, as the symptoms were by no means acute. Five hours after rupture the uterus and appendages were removed. The patient died within eight hours after the operation. At the necropsy the cause of death was found to be hemorrhage. The vascular and edematous pelvic connective tissue had shrunk up, so that the ligature had slipped. It is clear, says Backer, that mass ligatures are insufficient for the hypertrophied and vascular structures around a parturient uterus. Every divided vessel must be secured. The conjugate diameter of the pelvis was 3 7-10 inches.

ETHER VERSUS CHLOROFORM IN OBSTETRICS.

In a paper on "Some Things I Was Not Taught in Obstetrics," E. S. Bolland (*Boston Med. and Surg. Journal*, March 18, 1897) says a few words in praise of chloroform in midwifery practice. Chloroform he uses hundreds of times to once of ether. Boston, he says, justly proud of her great discovery of that great general anesthetic, ether, has not been fair to chloroform. He has not found that it predisposes to hemor-

rhage. As a routine practice he examines the heart before giving it, and under these restrictions he regards it as an ideal anesthetic during the latter part of the first stage and during all of the second stage of labor; it is seldom necessary during delivery of the afterbirth or for suturing a perineal tear.

TREATMENT OF UNCONTROLLABLE VOMITING IN PREGNANCY.

A. Pozzi (*Archivio di Ostet. e Ginecol.*, May and June, 1897) has treated successfully five cases of severe vomiting in pregnancy by the method proposed by Professor Titchener. In four of these the pregnancy had reached two and a half months, in one only a month and a half. The method referred to was the subcutaneous injection in the hypogastrium of a solution of hydrochlorate of cocaine (1 cg. to 1 g. of distilled water). In two of the cases the treatment was begun in the second stage of the vomiting, when there was fever and when cerebral phenomena had begun to manifest themselves. In two cases the cocaine was given when the vomiting was still in the first stage, and in the fifth patient the author had to do rather with an exaggerated form of simple vomiting than with the grave incoercible type. In all the cases a great number of other means of treatment—including in some replacement of the uterus—had been previously tried.

FACE PRESENTATION.

Gessner (*Centralbl. f. Gynak.*, No. 22, 1897) admits that in individual cases no satisfactory explanation of face presentation can be found. Obstetricians should report every case where fair evidence exists that some condition favors this presentation. In one of his own the patient was in her eleventh labor. He detected a spastic stricture in the lower uterine segment, which projected into the uterine cavity so as to hold back the fetal occiput. This spasm was suppressed by narcotics and warm applications, and the presentation was

spontaneously converted into the first vertex position, and delivery followed. This case suggested, in Gessner's opinion, Freund's theory that rheumatism of the uterus is a cause of face presentation. In a second case there was nothing remarkable at the labor; the child was born asphyxiated, and died on the next day. The cranium was markedly dolichocephalic, and the posterior arm of the head lever was nearly a centimetre longer than the anterior. As the deformity was one of the scaphocephalic variety it must have originated in the course of development in the uterus, and not during the process of labor. The case supports Hecker's theory. In the discussion on Gessner's paper Mullerheim supported Freund's theory. He had seen many cases like Gessner's first at Strassburg, where Freund had detected a relation between rheumatism and abnormal presentations, so much influenced by climate. Nagel and others supported Hecker's theory, having detected deformities of the occiput in face presentation.

TREATMENT OF PUERPERAL SEPSIS.

1. Suspected infection of the birth canal should be confirmed when possible by a bacteriological examination of vaginal secretions, and every

means of differentiating from other affections be resorted to, that they may be treated either by medicine or by surgery. 2. Irrigation and antiseptics destroy the nutrition of the parts when continued and, furnishing increased moisture, improve the field for the development of microorganisms, aside from the danger of death resulting from the antiseptic used. 3. The birth canal can be kept comparatively dry by absorbent dressing, removing the culture media and arresting the development of germs and infection until the abraded parts have healed.

—Miller, American Journal of Obstetrics.

MALIGNANT DISEASE OF UTERUS.

1. Cancer of the cervix uteri, if left without surgical interference, always kills. 2. The disease in most instances is primarily a local process. 3. Early hysterectomy will cure quite a percentage of these cases. 4. The microscope, while a great diagnostic aid, is not infallible in its findings. 5. The experienced surgeon is warranted in resorting to hysterectomy, even in doubtful cases. 6. Every malignant gravid uterus should be removed before the disease has advanced beyond the period of a probable cure.

—Cordier, Tri-State Medical Society, Chicago.

Miscellany.

THE TREATMENT OF CHRONIC RHINO-PHARYNGITIS.

R—Mentholi
 Oil of sweet almonds, or
 Liquid vaselinex
 M. S.—Apply locally with a brush.
 —Hamon de Fougerey.

ACETANILIDE POISONING IN A NEWLY BORN INFANT.

Irving M. Snow (Arch. of Pediat., June, 1897) has met with a case in which acetanilide crystals applied as an antiseptic to the stump of the umbilical cord caused symptoms of poisoning. About 60 grains were used and two days later (the ninth day of life) the infant became cyanotic, respiration 60, pulse quick and weak. There were no signs of congenital heart disease or of atelectasis pulmonum. The umbilical hollow was found to be filled with acetanilide powder, below which was a dry granulating surface with no signs of inflammation. Oxygen, whisky and digitalis were administered, but for ten hours no improvement took place; after 24 hours, however, the child slowly recovered, the cyanosis having lasted for 72 hours. There was a loss of one pound in the body weight in three days. Snow has collected from literature a number of cases of acetanilide poisoning in young children.

ICHTHYOL IN BLEPHARITIS.

Darier (Clin. Ophthalmologique, February 10, 1897) recommends the following ointment for application in cases of blepharitis: Ichthyl 0.50 cg., powdered starch 5 g., oxide of zinc 5 g., vaseline 25 g. Its use has to be continued for some time, alternating if necessary with the application of silver nitrate, etc. In bad cases a little pure ichthyl may be smeared on the swollen lid margins at night, the lids being well washed with boric acid lotion in the morning. While ichthyl diminishes congestion it does not seem to diminish the conjunctival secretion, and where this is marked silver nitrate is called for. In rebellious phlyctenular keratitis with much vascularization, Darier has obtained good results by introducing daily a minute portion of pure ichthyl into the conjunctival sac. This causes some smarting, which, however, passes off in about ten minutes.

INTRAVASCULAR INJECTIONS OF SUBLIMATE AND QUI- NINE.

D'Anna (Il Policlinico, March 1, 1897) has been experimenting on dogs, with solutions of sublimate (1 in 100 and 1 in 1000 of NaCl solution) and bichloride of quinine (1-2 g. to 2 g. of water). The effects of injecting one or other of these solu-

tions into arteries or veins, against or with the current of blood and also with varying degrees of force, were carefully investigated. With regard to quinine, no matter what the technique or whether it is injected into a vein or an artery, no coagulation takes place. There is no danger in injecting quinine. The case is different with regard to mercury; solutions of sublimate (1 in 100) are absolutely dangerous, 1 in 1000 solu-

tions cause changes in the vessels (thrombi, endothelial destruction, alteration in the muscular coat), which should theoretically cause damage, although clinically the result is not so deleterious. Possibly the results obtained in dogs may differ from those which have been observed in men. Experiments with cyanide of mercury (1 in 1000) gave the same results as those with perchloride.



Book Reviews.

TYPHOID FEVER AND ITS ABORTIVE TREATMENT, BY JOHN ELIOT WOODBRIDGE, M. D.—THE CLEVELAND MEDICAL PUBLISHING CO., NO. 48, THE ARCADE, CLEVELAND, O., PUBLISHERS.

This work, which is the compilation of a number of papers, written by Dr. Woodbridge on the subject of typhoid, and contains the now celebrated "Woodbridge Treatment of Typhoid," comprises 338 pages of the etiology, pathology, symptomatology and new treatment of this disease. We agree that the doctor has, in our estimation, the right idea in the treatment of typhoid; i.e., early elimination and disinfection. We fail, however, to see what great amount

of importance can be attached to the slight variations of formulas Nos. 1, 2 and 3, or why either of them may not be all that is practically required in attempting to abort the disease. It has always seemed rational to suppose that primarily typhoid is a local infection, and should, within a limited period, be aborted by eliminative and antiseptic treatment. The book is well worth the small price, and full of practical suggestions, whether the treatment will hold fire or not. One thing is certain in all new methods, and that is, no severe criticism should be made without a faithful trial under the specific directions as laid down by the author. If this treatment holds good a great advance has been made by Dr. Woodbridge in medical science.